ON TIME JUDGMENT.

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§ 1. THE QUESTIONS STUDIED.

The following experiments are concerned with two questions regardiug Time Judgment.

I. What "filled" period of Time can be most accurately estimated?

II. When two filled periods of different duration are given is the duration of the period which is estimated as midway between them, the arithmetic or the geometric mean of the two periods?

The experiments dealt only with "filled" periods, *i. e.*, periods coloured by a continuous sensation of specific quality, these being in the opinion of the writer psychologically simpler than the so-called "empty" periods where there is no definite colouring, but a mingling of fleeting images and passing thoughts with organic and muscular sensations. Whatever be the data upon which Time Judgment is based, homogeneity in the character of the period would seem to favour estimation. The "filling" selected was that of sound. As grounds for the selection, two points may be noticed. (1) It is fairly easy to produce a period of sound which is homogeneous in character. (2) Sound has the advantage over Colour or Brightness of having sharper termini *a quo* and *ad quem* and is freer from after-image effects.

As far as the writer is aware the first question has not been made the special subject of investigation. Meumann's investigation (*Phil. Studien*, Bd. 12, 2 H.) which deals with the estimation of empty in comparison with "filled" periods and the influence of various kinds of filling upon the estimation, is closely allied to it. Schumann (*Zeitschrift f. Psy. u. Phys. der Sinnesorgane*, Bd. 4) has dealt with the question for

empty intervals. The second question is included in an investigation carried out by F. S. Wrinch ("Ueber das Verhaltniss der ebenmerklichen zu den übermerklichen Unterschieden im Gebiet des Zeitsinns," Wundt's *Phil. Studien*, 18 Bd. 2 H). Although that investigation has a different purpose, a wider scope, and a far more elaborate method than that of the present series of experiments, yet in so far as it treats of the same question, it shows general agreement with the results given below.

§ 2. Method of Experiment and Arrangement of Apparatus.

The experiments were carried out in the Physiological Laboratory of the London University, continuing from May, 1902, until March, 1903.

The method of experiment used was the method of Average Error or, more descriptively, the method of Reproduction.

In Investigation I, the period which could be most accurately estimated was determined by the period which the subject could most accurately reproduce after its delivery.

Similarly in Investigation II, the subject's estimation of the mean between two given periods was determined by the period which the subject produces as his judgment of the mean period.

To carry out this method the following arrangement of apparatus was made. (See Fig. 1, p. 156.)

The subject of the experiment is placed in a room (Room 2) away from the recording apparatus and other distractions, and receives a sound, produced by induction currents at 50 per sec. in Room 1, by means of a telephone, in Room 2, attached to the secondary coil.

The duration of this sound is registered in Room 1 upon a kymograph by means of a Pfeil's signal inserted in the primary circuit.

This circuit can be completed by two paths. (1) It is completed automatically by the trailing of a spring (k_1) against a wheel bearing contact strips which is fixed on the rotating axis of the kymograph.

(2) It can be completed by a key in the hands of the subject, (k_2).

At every rotation of the contact wheel, the subject receives a sound of given duration regulated by the length of the con-

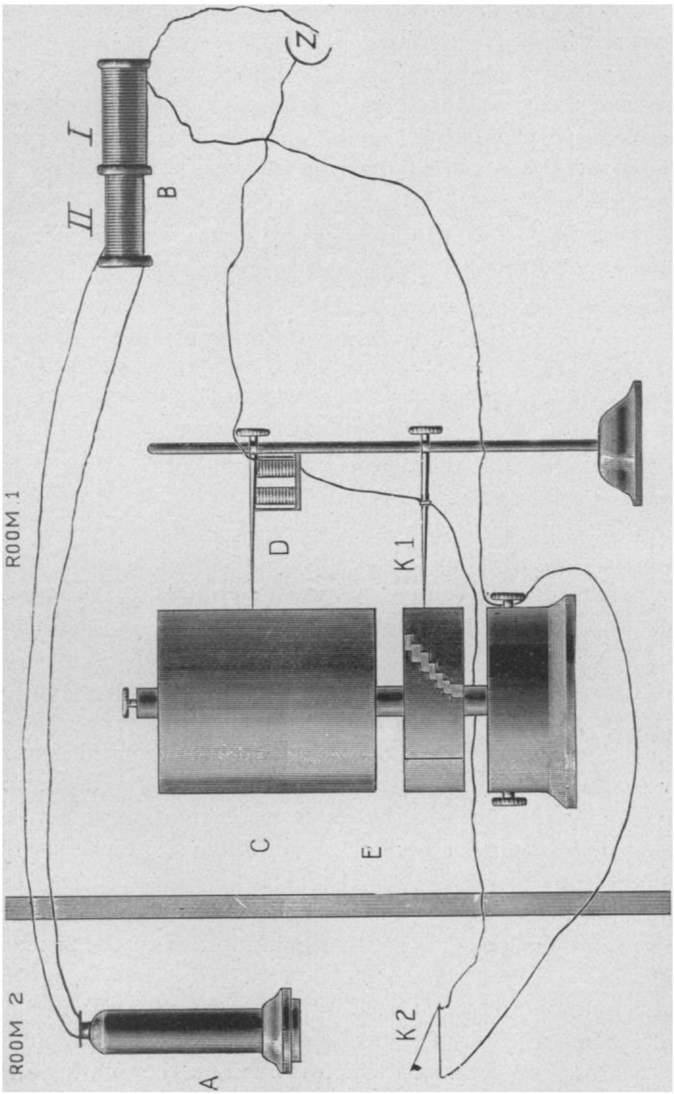


FIG. 1.

tact strip to which the spring is adjusted and the rate of rotation. By closing his own key the subject reproduces in the

telephone a period of sound which seems to him equal in duration to that just delivered.

Both the standard, or 'Question,' duration and its reproduction, or 'Answer,' are registered by the magnetic writer upon the drum of the kymograph.

Throughout any one series the standard period is usually kept constant, variations being only introduced now and again to test the subject's attention; but the series themselves follow no regular gradation, long periods being given after short ones or *vice versa*.

In experiments to ascertain the mean, the subject receives two periods of sound, the strips upon the contact wheel being arranged in pairs. No regular order is followed in the two delivered periods. In one series the two periods may be in the order short-long, in the next series in the reverse order, long-short, if the subjects of the experiment have been tested for any constant error due to order.

The figures below (Figs. 2 and 3) give reproductions of two records.

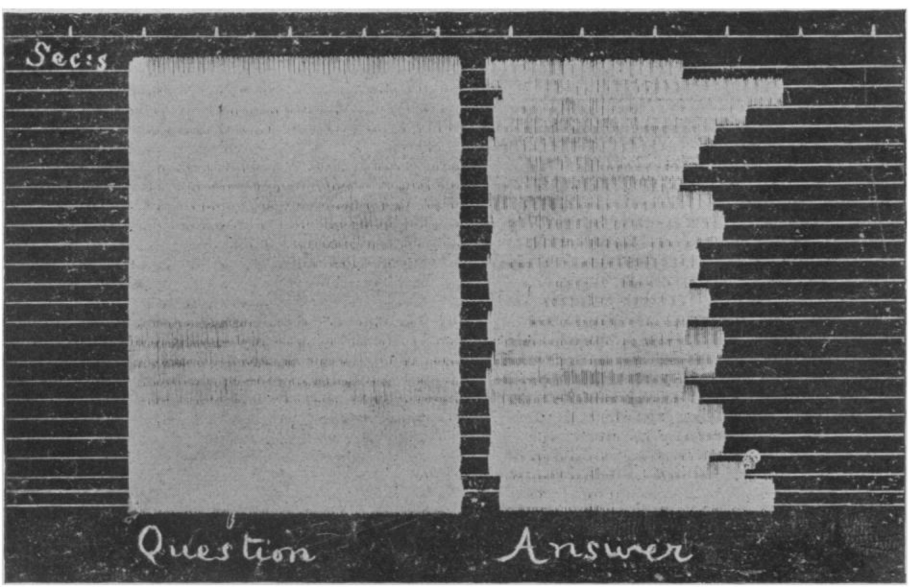


Fig. 2. Series of experiments wherein the subject sought to reproduce a period equal to the period delivered. (Subject S. Table I.)

PROTOCOL FOR FIG. 2.

Question		Answer	M. Variation
	1	36·25	6.81
34 mm.	2	36·5	7·06
	3	29·25	·19
$= 3502\ \sigma$	4	31	1·56
	5	30·5	1.06
	6	27·5	1·94
	7	26	3·44
	8	29·75	·31
	9	30	·56
	10	26·25	2·19
	11	28·75	.69
	12	26·25	3·19
	13	28	1·44
	14	27	2·44
	15	30·5	1.06
	16	29·5	.06
	17	25	4·44
	18	25·75	3·69
	19	28	1·44
	20	29·25	·19
	21	33·5	4·06
	22	37	7·56
	23	25·75	3·69
Total		677·25	60.07
÷ 23		29·44	2.61

Rate of Rotation

9·75 mm. = 1 Sec.
1 mm. = $103\ \sigma$

Mean Estimation

29·44 mm.
= $3032\ \sigma$

Mean Variation

2.61 mm.
= $269\ \sigma$

Error. —$470\ \sigma$

The kymograph used in my experiments was that by Sandström (Lund) which is driven by an electric motor in the base of the instrument. Attached to the motor is a regulator by which the rate of rotation can be varied from .05 mm. to 1000 mm. per sec. Throughout the whole period of experiment the instrument ran with the greatest smoothness and accuracy, giving whenever tested the most satisfactory results. When tested half way through the whole term of experiment, the drum showed a mean variation of $16\ \sigma$ per sec. at the 50 mm. per sec. rate of rotation, the rate used in the majority of the experiments.

Below (p. 160 [424]) is a cutting of a tracing given with Jacquet's Time Marker at the two speeds used, viz. 50 mm. per sec. and 10 mm. per sec. The straightness of the align-

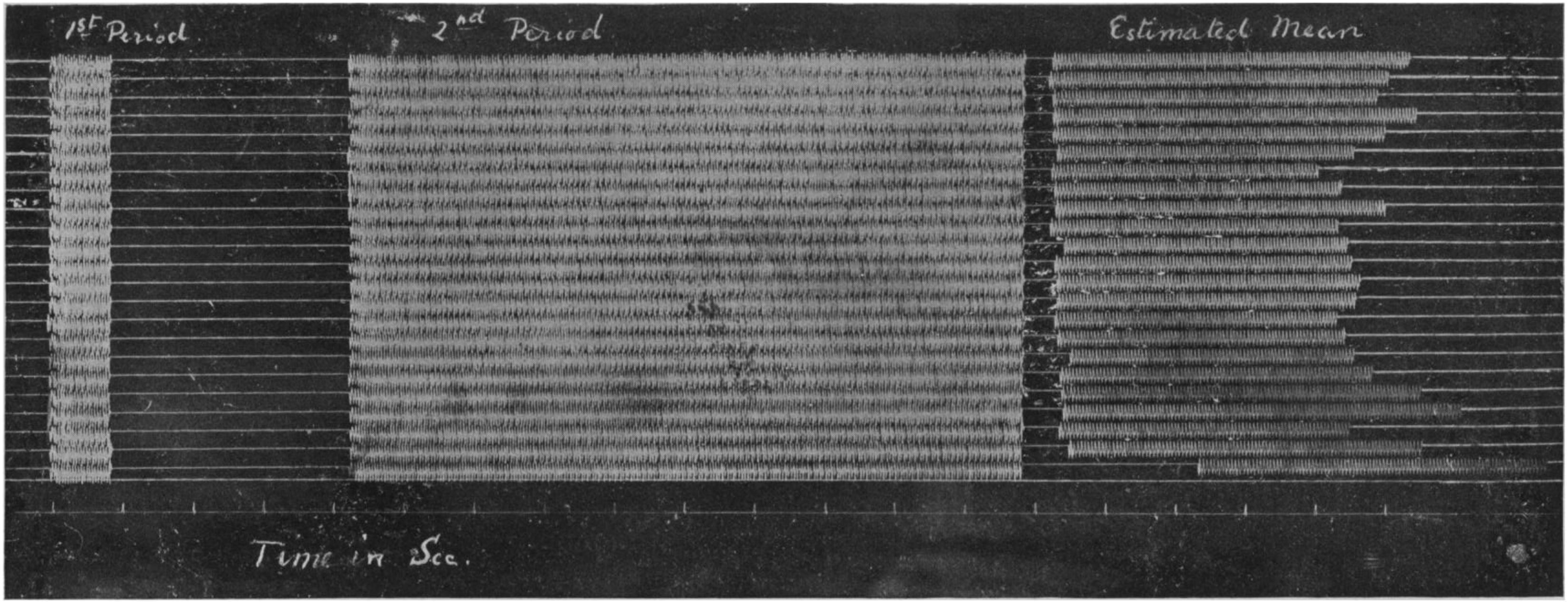


Fig. 3. Series of Experiments wherein the subject sought to produce the mean of the two delivered periods. Subject S. Tables II (c).

PROTOCOL FOR FIG. 3.

Periods given		Estimated Mean	M.Variation
8·5 mm.	1	48·75	4·45
and	2	49·25	4·95
94 m.	3	·40·25	4·05
= 875 σ	4	56	11·7
and	5	50	5·7
9682 σ	6	43·5	.8
	7	49·5	5·2
	8	40	4·3
	9	39·5	4·8
	10	41·75	2·55
	11	42·25	2·05
	12	41·5	2.8
	13	39·75	4·55
	14	50	5·7
	15	45·75	1·45
	16	39·75	4·55
	17	37	7·3
	18	41·25	3·05
	19	41	3·3
	20	40·5	3·8
	21	45	·7
	22	47	2·7
	23	49·75	5·45
Total		1019·	95·9
÷ 23		44·3	4·17

Rate of Rotation

9·75 mm. = 1 Sec.
1 mm. = 103 σ

Average Estimated Mean of given Periods

44·3 mm.
= 4563 σ

Mean Variation

4·17 mm.
= 429 σ

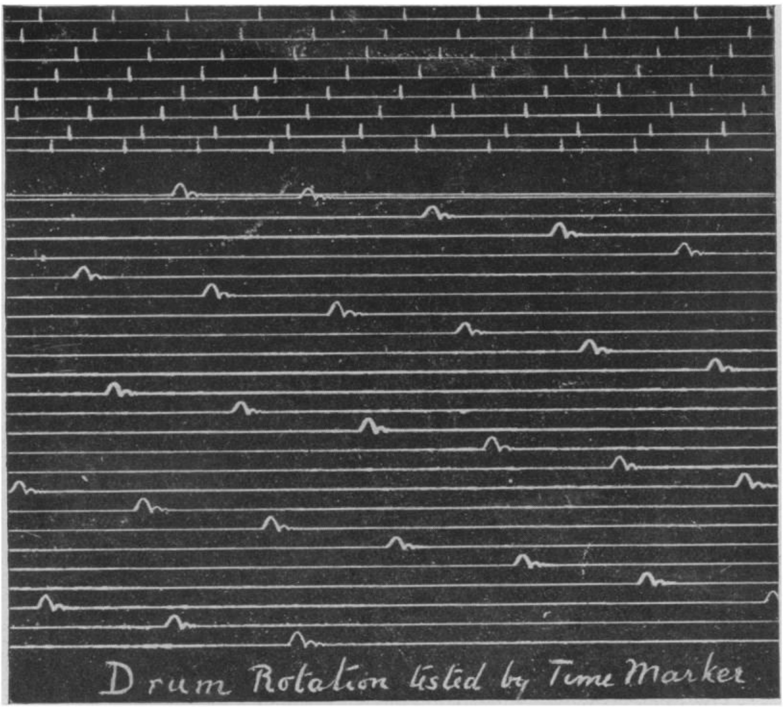
Arithmetic Mean 5278 σ
Geometric Mean 2911 σ
Error for A. M.—715
" " G. M. + 1642

ment of time-marks on the successive lines of revolution of the cylinder (which descends on a spiral screw) affords a very exacting test of constant speed of revolution. The records of August 1st, at the outset of a series of experiments, and of Oct. 18th, at the close of a series, are practically indistinguishable.

§ 3. RESULTS.

I. In the first investigation three persons, S., B., and Sp., acted as subjects for systematic series of experiments. In the case of subject B., owing to the impossibility of further attendance, the series is less complete than is desirable.

There was no agreement shown in the period found favourable to estimation, this being 3.33 sec., 1.94 sec. and 1.07 sec. for the three subjects S., B. and Sp. respectively.



For each subject, however, it was found that periods longer than the favourable period were *under*estimated, those shorter than the favourable period were *over*estimated.

The details of the results are best shown by the following tables and curves.

TABLE I, FOR SUBJECT S. *Value in σ.*

Q.	A.	Error.	M. V.	M. V. %
255	594	+339	139	23.4
459	750	+291	91	12.13
673	913	+240	55	6
857	1122	+265	112	10
918	1234	+316	100	8.1
1040	1339	+299	128	9.56
1265	1462	+197	68	4.6
1390	1730	+340	251	14.5
1520	1742	+222	99	5.7
1673	1887	+214	141	7.5
1714	1964	+250	109	5.5
1918	2103	+185	71	3.35
2472	2650	+178	167	6.3
3348	3354	+ 6	212	6.3
3502	3032	—470	264	13.4
4532	3430	—1100	608	17.7

Q. = Period delivered. A. = Subject's estimation of the same.
M. V. = Mean Variation. Total number of experiments, 290.

Value in σ.

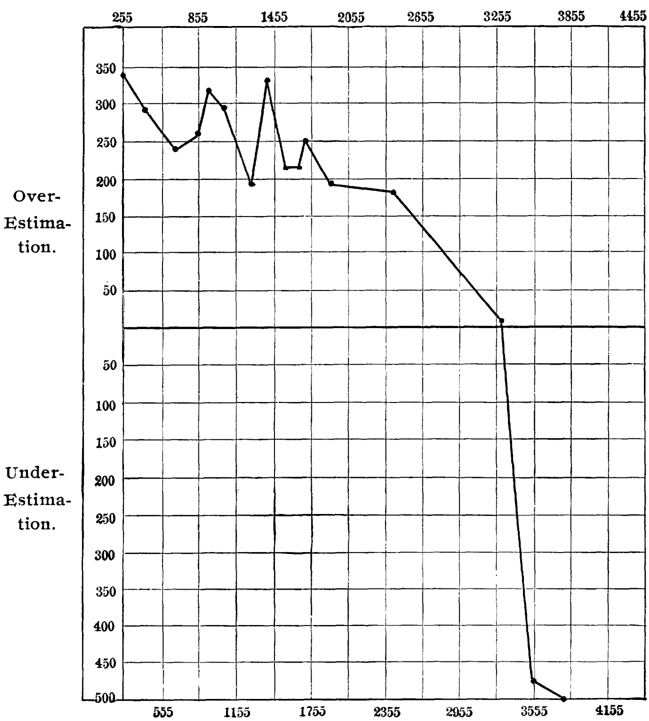


FIG. 4. Curve of Error for Subject S. from the above table.

TABLE I, FOR SUBJECT B. *Value in σ.*

Q.	A.	Error.	M. V.	M. V. %
257	319	+ 62	159	62.25
340	509	+169	71	20.88
515	653	+138	71	13.78
649	801	+152	87	13.4
659	853	+194	108	16.38
886	1154	+268	89	10.04
1000	1491	+491	123	12.3
1071	1281	+210	75	7
1257	1469	+212	116	5.42
1586	1700	+114	200	12.61
1710	1973	+263	202	11.81
1963	1949	— 14	121	6.16
3863	3707	—156	102	2.64
6522	6233	—289	521	8

Q. = Period delivered. A. = Subject's estimation of the same. M. V. = Mean Variation. Total number of experiments, 444.

Value in σ.

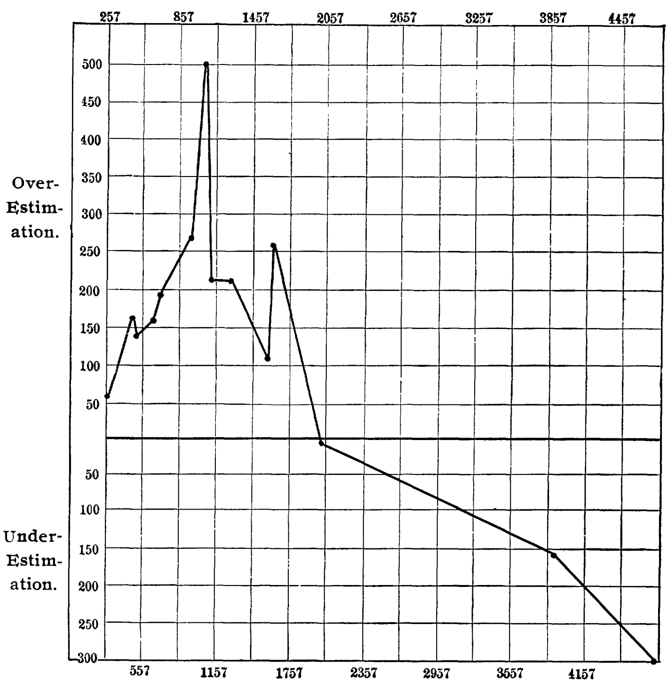


FIG. 5. Curve of Error for Subject B. from the above table.

TABLE I, FOR SUBJECT SP. *Value in σ.*

Q.	A.	Error.	M. V.	M. V. %
268	389	+121	45	17
474	518	+ 44	41	8.65
680	679	— 1	39	5.73
700	746	+ 46	43	6.14
886	876	— 10	90	10.15
1071	1081	+ 10	80	7.46
1318	1251	— 67	74	5.61
1463	1396	— 67	102	7
1931	1686	—245	67	3.46
2369	2206	—163	91	3.84
2421	2367	— 54	126	5.2
3502	3340	—162	116	3.31
4528	4356	—172	275	6.07

Q. = Period delivered. A. = Subject's estimation of the same.
M. V. = Mean Variation. Total number of experiments, 423.

Value in σ.

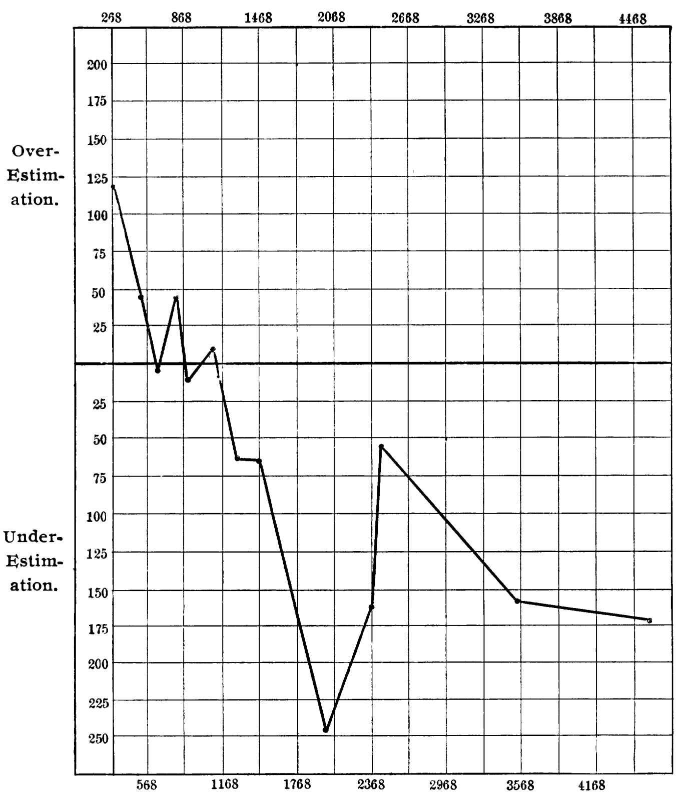


FIG. 6. Curve of Error for Subject Sp. from the above table.

Besides the three systematic series given, single sittings of first trial experiments were taken with the subjects W., G., Wy. and R.

The results of these tend to confirm the general rule quoted above, showing *over*estimation in the case of short periods and *under*estimation in the case of long.

TABLE I, FOR FIRST TRIAL EXPERIMENTS ON SUBJECTS W., G., WY. AND R.

Subject	Q.	A.	E.	M. V.
W.	886	1290	+404	107
	1936	2194	+258	135
G.	494	566	+ 72	45
	700	740	+ 40	41
	1061	1097	+ 36	75
	2369	2402	+ 33	223
	3450	2910	—540	772
Wy.	906	1080	+174	100
	1926	2075	+149	154
	3399	3430	+ 31	289
R.	711	849	+138	103
	896	954	+ 58	113
	3450	2831	—619	344

II. In the second investigation systematic series were made with the two subjects S. and Sp.

Speaking generally, the period judged to be midway between the two given periods was nearer the Arithmetic than the Geometric Mean, a result in conflict with Weber's Law.

With short periods, having, roughly, ratios from 1:8 to 1:6, the Estimated Mean is even greater than the Arithmetic Mean. (Tables II a.)

With longer periods, having the same ratios, the Estimated Mean is less than the Arithmetic Mean. (Tables II c.)

Greater approximation to the Geometric Mean than to the Arithmetic Mean occurs in those cases where the Geometric Mean falls near the "favourable" periods of Investigation I, or where the ratios between the given periods are very small. In experiments with subject Sp. these two conditions fell together. (Tables II b.)

TABLES II, FOR SUBJECT S.

(a) Periods given short with large Ratio.
Estimated Mean greater than Arithmetic Mean.

Value in σ.

R.	Periods	A. M.	G. M.	E. M.	Error A. M.	Error G. M.	M. V.	M. V. %
$\frac{1}{7.1}$	265, 1897	1081	709	1600	+519	+891	213	13.3
$\frac{1}{7}$	265, 1877	1071	705	1259	+118	+554	113	9
$\frac{1}{6.7}$	286, 1938	1112	744	1943	+831	+1199	175	9

R. = Ratio. A. M. = Arithmetic Mean. G. M. = Geometric Mean.
E. M. = Estimated Mean. M. V. = Mean Variation.
M. V. % = Reckoned on the Estimated Mean.
Number of Experiments, 284.

(b) Periods given short with small Ratio.
Estimated mean less than Arithmetic Mean. In *two* cases nearer Geometric than Arithmetic Mean.

R.	Periods	A. M.	G. M.	E. M.	Error A. M.	Error G. M.	M. V.	M. V. %
$\frac{1}{3.7}$	473, 1751	1112	910	1551	+439	+641	111	7.15
$\frac{1}{3.6}$	469, 1693	1081	891	1530	+449	+639	215	14
$\frac{1}{3}$	565, 1709	1137	983	1102	− 35	+119	101	9.15
$\frac{1}{1.5}$	844, 1318	1081	1055	934	−147	−121	184	19.7
$\frac{1}{1.3}$	927, 1277	1102	1088	1017	− 85	+ 71	131	13

Number of Experiments, 123.

(c) Periods given long with large Ratio.
Estimated Mean less than Arithmetic Mean. In *three* cases nearer Geometric than Arithmetic Mean.

R.	Periods	A. M.	G. M.	E. M.	Error A. M	Error G. M.	M. V.	M. V. %
1/11	875 – 9682	5278	2911	4563	− 715	+1652	429	9.4
1/8.5	1133 – 9682	5407	3312	3770	−1637	+ 358	268	7
1/7	1339 – 9579	5459	3581	4424	−1035	+ 843	380	8.6
1/6.5	1442 – 9476	5459	3697	3203	−2256	− 494	340	10.6

Number of Experiments, 76.

TABLES II, FOR SUBJECT SP.

(a) Periods given short with large Ratio.
Estimated Mean greater than Arithmetic Mean.

Value in σ.

R.	Periods	A. M.	G. M.	E. M.	Error A. M	Error G. M.	M. V.	M. V. %
1/8.6	227 – 1957	1092	667	1175	+83	+508	74	6.3
1/7.2	268 – 1936	1102	720	1170	+68	+450	102	8.6
1/6.7	144 – 968	556	373	648	+92	+275	94	14.5
1/6.2	309 – 1936	1123	774	1151	+28	+377	97	8.4

R.=Ratio. A. M.=Arithmetic Mean. G. M.=Geometric Mean.
E. M.=Estimated Mean. M. V.=Mean Variation.
M. V. %=Reckoned upon Estimated Mean.
Number of Experiments, 113.

(b) Periods given short with small Ratio.
Estimated Mean less than Arithmetic Mean. In *two* cases nearer Geometric than Arithmetic Mean.

R.	Periods	A. M.	G. M.	E. M.	Error		M. V.	M. V. %
					A. M.	G. M.		
$\frac{1}{4}$	474, 1936	1205	958	1243	+ 38	+285	86	7
$\frac{1}{1.5}$	844, 1318	1081	1067	962	—119	—105	77	8
$\frac{1}{1.1}$	1112, 1277	1195	1192	1065	—130	—127	74	7

Number of Experiments, 78.

(c) Periods given long with large ratio.
Estimated Mean less than Arithmetic Mean, but in *no* case nearer Geometric than Arithmetic Mean.

R.	Periods	A. M.	G. M.	E. M.	Error		M. V	M. V. %
					A. M.	G. M.		
$\frac{1}{8}$	1210, 9682	5446	3423	5223	—223	+1800	194	3.7
$\frac{1}{7}$	1339, 9579	5459	3581	5792	+333	+2211	381	6.5

Number of Experiments, 47.

§ 4. The Psychological Import of the Experiments.

I. The fact brought out by the first set of experiments is that short durations ($\frac{1}{5}$ — 1½ sec.,) are *over*estimated while longer durations (2—4 sec.,) are *under*estimated.

What psychological explanation can be offered for the *over* and *under*estimation of short and long periods respectively?

To seek any explanation demands a close analysis of the influences under which the subject makes his estimation. Many of the circumstances are what one may properly call accidental, variable from one experiment to another, one subject to

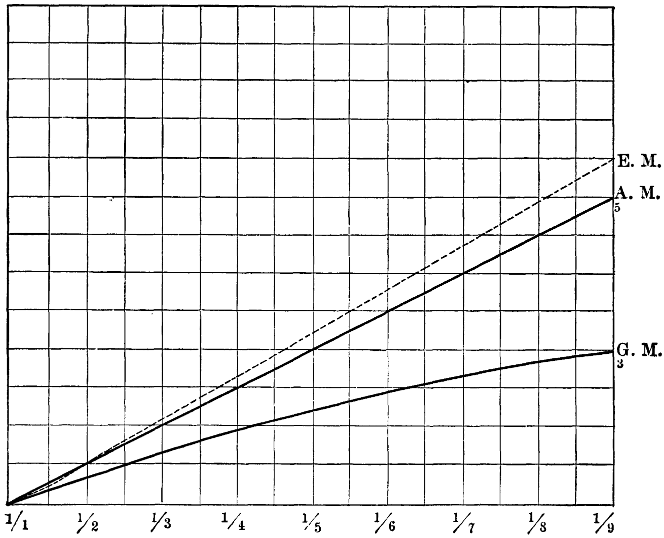


FIG. 7. Diagram showing the relation of estimated mean to A. M. and G. M. at various ratios. (Based on tables for Subject S.)

another, and without influence, therefore, on the average general result. Other circumstances are causal, but may, nevertheless, be peculiar to the method of experiment adopted and not essential to the estimation as such. It is the separation of these from the essential conditions which is difficult.

(a) The procedure was in a special sense procedure without knowledge. In his estimation and imitation of the duration the subject could rely only upon his actual experiences. There was no understanding between experimenter and subject that the period to be estimated should be kept the same throughout any series of experiments, though it usually was so, in order that any improvement in estimation due to Practice might be seen and taken into account. This entire absence of knowledge is not possible where a Gradation Method is used. There the subject knows that of the two durations given him, one is longer than the other. He knows further that there will be gradual change until equality is reached, and then further change until the original relations are reversed. The effect of such knowledge, like that of its absence, is seen, not in the general nature of the judgment, but in its degree of delicacy and in the con-

stancy of its pronouncement. With an Error Method, therefore, one must expect a larger Crude Error and a larger Mean Variation than in a Gradation Method.

(b) Error due to Position could not be influential, since only one period was delivered to the subject. By choosing the length of the period for each series of experiments with as much irregularity as possible, the error of Position was eliminated between series and series.

(c) So far the conditions of method considered have been negative. It was a positive condition peculiar to the method that the estimation took place between a memory image and an actuality. The subject imitated his memory image. The contrast between the idea and that which is actualizing itself in the present might be expected to have some influence upon the estimation; but the influence, whatever it may be, should be present in all estimations alike, and will not, therefore explain the *over*estimation of short intervals and the *under*estimation of long.

(d) It was also a positive condition that while making his estimation, while imitating his memory image, the subject was receiving muscular sensations from his operations on the key, which sensations might have influenced his estimation. This circumstance, however, would be present in both short periods and long, and if causal, would have a constant tendency in both alike.

The essential conditions of *over* and *under*estimation must vary with long and with short periods, since different effects mean different causes. One such set of conditions might be Dilatation or Contraction of the Memory Image. If it were a law that Memory Images dilated or contracted with lapse of time, so as to become of longer or shorter duration than their originals, the difference in time which must elapse between the beginning of the formation of the image and its imitation in the case of long and short intervals respectively, would influence an estimation based on Memory Images by rendering these Images of unequal growth or of unequal shrinkage.

Some separate experiments were made upon this point, but no law of dilatation or contraction could be deduced. Even if such conditions were at work, their effects in so brief an exist-

ence of image could not be sufficiently appreciable to explain the differences in estimation.

The differences between the two sets of periods, short and long, is so restricted that it seems right to reject as insufficient any explanation based solely upon an analysis of the sensations and after-images received. The question here is not primarily upon what data is Time Estimation based, but what are the conditions which make the estimation so different in the case of short and longer periods respectively. The ground of explanation seems to the writer to be in another direction. The usual predication of Duration as an attribute of sensation on all fours with Quality and Intensity, is apt to obscure the fact that, properly speaking, for Psychology Duration belongs only to the Dynamic view of Consciousness, *i. e.*, to Consciousness viewed as a stream, as a life of interconnected processes. To predicate Duration of an isolated sensation, is, from the *subjective* standpoint of Psychology, unthinkable. It may be said, 'This is a metaphysical difficulty,' but is not rather the contrary true? It is the metaphysical standpoint which makes us predicate Time of the element which we are avowedly considering in abstract. Duration must be the abject creature of the Law of Relativity whatever partial freedom Quality and Intensity may claim. It would seem, therefore, that it is in the Dynamic view of Consciousness that the explanation for the phenomenon of *over* and *under*estimation should be sought.

Rarely is the stream of our conscious life so narrow that we can detect in it only a single process or current. Our estimation of Duration is bound up with this plurality. We measure process against process; one endures, others come and go, this is long, those short. Should we be conscions of one current alone, *e. g.*, the interesting story our favourite novelist is unfolding to us, we lose count of time, and only realize it subsequently by noting other currents which have flown on unnoticed beside it and which we now recover, or by an appeal to some objective standard. In the experiments under consideration, every effort is made, as a preliminary step, to narrow the stream of consciousness. The subject is asked to concentrate his attention on the task before him, he waits especially

for the sound sensation. When it comes what happens? As far as the writer is able to analyze the situation, it seems that with a sound of short duration, the sound is the sole object of consciousness. Consciousness is through the concentration of attention for one brief interval dammed into a single current, it is all sound. For the after consciousness such an occurrence is a big event. There are not recoverable any temporarily unnoticed processes running along side of it, with which it can be compared. It stands in "splendid isolation" separating the before and after, as far as moments of a stream can be separated, and through its isolation it is *over*estimated.

With longer duration the situation is different. There is a limit to concentration. Consciousness is not all sound, other processes flow on beside the sound; muscular and organic sensations, fleeting images, passing thoughts, are present to consciousness. The process has relation to other processes, and the greatness which comes from mere absence of comparison is lost to it, thus there is no ground for *over*estimation. But through the presence of these other processes, it not merely loses its abnormal proportions, but suffers actual diminution. There has been diversion of attention and hence loss in mental value, so that the sound process seems to consciousness less than what its objective value warrants.

Against this attempt at analyzing the situation, the charge may be brought that it attempts to explain by having recourse to that which itself stands most in need of explanation—attention. That is true, but it is at least one step in the analysis if one can realize what function this *Deus ex Machina* performs.

II. The second set of experiments brings out the fact that Duration does not obey Weber's Law. The interest in ascertaining whether Duration does or does not lie under the great Sensation Law, lay partly in the indication this might give of the similarity or dissimilarity between Duration and Quality or Intensity, but chiefly in its connection with a theory of psychological measurement.

The question whether it is possible to have a system of psychological measurement must always have a methodological, if not a practical interest for psychologists. Ebbinghaus in his recently published "Grundzüge der Psychologie" sets forth

a system which is founded directly upon Weber's Law in such a way that the difficulties of Fechner's unit are avoided. The unit for measurement is "a directly experienced difference or distance between two sensations of the same kind, which are compared with one another in any respect." (Grundzüge, S. 63.) That we can perceive equality of differences is for Ebbinghaus a fact. "The equality of the steps here is no hypothesis or conventional assumption." (*Ibid.* S. 502.) Given a unit difference we can measure by means of it the difference (in either a positive or a negative direction) between any one sensation chosen as zero and any other.

The equal differences between sensations correspond to a Geometrical Progression between stimuli. Hence the formula. "The Intensity of sensations grows proportionally to the log. of the corresponding stimuli." (*Ibid.* S. 509.)

If the clearly perceptible differences which bear this relation to stimuli are known to be equal, then the least perceptible differences which bear the same relation must also be equal. That is, Fechner's hypothetical unit of measurement is to be accepted as valid practically, being deduced as a consequence of Ebbinghaus's empirical unit.

In this way Ebbinghaus saves the Fechner hypothesis from rejection, and yet at the same time does not use it as foundation for a system, a function which the results of recent experimental research seem to show it incapable of fulfilling. (Külpe, "Ueber das Verhältniss der ebenmerklichen zu den übermerklichen Unterschieden," Paris Congrès 1902. Ament, *Phil. Studien*, Bd. 16. S. 180.) The system, however, is founded upon what appears to the writer an unwarranted assumption, viz., that equality of sensation differences is something empirically verifiable.

Weber's Law gives us a certain functional relationship between stimuli and sensations—nothing more; the psychological unit needs independent proof. That the differences corresponding to a Geometrical Progression of stimuli are equal has to be demonstrated. Ebbinghaus cites the judgment of painters and embroiderers as to equal differences of shade. There is no doubt that such people term differences equal or multiples of each other, but what do they mean by it? It seems as though

ultimately they mean one of two things—either they mean that objectively, with reference to the amount or strength of pigment, there is equality or multiplicity, or, and this is the commoner case, they mean that they can imagine the same number (or a multiple of that number) of intermediate steps in passing from one shade to the other. If pressed as to their warrant in supposing these intermediate steps equal, they would finally take refuge in least perceptible differences, *i. e.*, we should be back at Fechner's hypothesis and must reverse Ebbinghaus's deduction of equality. Fechner himself seems to have recognized that it was the equality of least perceptible differences which was fundamental for the equality of the clearly perceptible. "Die Unterschiedsmassformel *ist als das Allgemeinere* der Unterschiedsformel, und hiemit auch der Massformel und Fundamentalformel anzusehen, sofern diese selbst besondere Fälle der Unterschiedsformel sind." (Elemente der Psychophysik, II, p. 103.)

If one considers what a difference between sensations in their Intensity or Quality is subjectively, one may the more easily realize how difficult it is to give a meaning to the "equality" of such differences, and echo the words of Kries, 'A measurement of intensive quantities will not be theoretically possible, until it is arbitrarily determined what is understood by "equality." (Kries, *Vierteljahrschrift für wissenschaftliche Philosophie*, 1882. p. 352.)

We may allow with Ebbinghaus that the difference between quality x and quality y, or intensity x and intensity y, is sensational, *i. e.*, directly bound up with the sensations of the qualities or intensities x and y, but it will not itself be a sensation or a quality or intensity of sensation, and what is meant by its being 'equal to' or 'a multiple of' another difference between sensations, is hard to see.

It was this difficulty which suggested the experiments upon Duration. Suppose Duration to be an attribute of sensation. Then since differences between sensations in respect of this attribute are—both from the objective and subjective standpoint—Duration, equality of such differences would be intelligible. Here, at least, therefore, was a direction where Ebbinghaus's unit for measurement might be used.

If estimation of Duration showed agreement with Weber's Law, then a measurement system according to the well-known formula, $E = k \log. R.$ would be possible.

Since estimation of Duration does not follow Weber's Law, it seems to the writer that Ebbinghaus's system of measurement fails to have validity for any aspect of sensation.